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Decomposing the impacts:
Lessons from a multistate analysis of enterprise zone programs

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Abstract

This paper exploits the exogenous variation of the U.S. state enterprise zone policies to estimate the impact of geographically-targeted tax incentives on a number of dimensions of local economic growth. The econometric analysis uses plant-level data from 11 state programs to sort out growth outcomes into gross flows separately accounted for by new, existing, and vanishing establishments in the target areas. The paper extends the literature by moving beyond a dichotomous treatment indicator to incorporate the contribution of a number common zone policy features. Although the findings of no net mean impacts of the zone programs on various measure of growth is consistent with previous research, the disaggregation into various gross flows and examination of the heterogeneity of policy implementation shows that the impacts of the incentives are more complex. Such analysis also lends itself to a more useful set of policy recommendations.

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1. Introduction

During the past quarter century, geographically targeted tax incentive programs have become an increasingly popular method to administer economic development initiatives by focusing on attracting and retaining firms in specific economically blighted areas. Following the UK experience from the early 1980s (McDonald, 1995), a substantial majority of US states have implemented economic revitalization programs, mainly referred to as enterprise zone (EZ) programs. Twenty-five years later, states are now re-visiting their programs, trying to determine whether and how to proceed with these incentives. Partially because the federal government did not implement a national program until 1994 (HUD, 1999), comprised initially of “Empowerment Zones” and “Enterprise Communities,” the states implemented programs that varied considerably in terms of designation criteria, geographic size of the program, and incentives offered (HUD 1997). Such variety is a precious resource, as it stems from political decisions that are likely uncorrelated to future economic trends in the target areas. These natural experiment conditions favor comparative analysis to test the effectiveness of different implementation features as best practices for future interventions. EZ areas are also small enough that appropriate comparison areas can be found within the same regional economies. Thus, program impacts can be estimated from empirical models that control for factors contributing to the observed outcomes that are independent from the program intervention. State EZ programs therefore have the potential to offer valuable empirical evidence on the effectiveness of geographically targeted tax incentives and to contribute evidence regarding the longstanding debate on the effect of tax differentials on firm location decisions.

Unfortunately, findings from the existing enterprise zone literature are difficult to translate into useful policy recommendations. One problem is the inconsistency of previous results. Studies have used various methodologies, including descriptive case studies and careful econometric evaluations.

Many of the evaluations have examined only one state's programs, and a few have evaluated a single zone. Conflicting results underscore the inability to generalize the empirical evidence of EZ studies. Findings are particularly hard to generalize because of the variety of EZ approaches and local economic conditions, but many impact evaluations only consider the presence or absence of the program in a particular time and place rather than taking into account the policy features of the zone or the generosity of tax incentives (Peters and Fisher, 2002). While findings from such studies are useful for helping evaluate whether to continue or terminate a particular program, they offer few actionable recommendations for improving the programs and they offer few hints as to whether the programs may be effective in other locations.

This paper contends that an effective way to evaluate state EZ programs is to use establishment-specific panel data across multiple states to comparatively estimate the impact of state-specific policy implementation features on gross flow measures of economic growth. That is, in addition to examining net changes in the economic outcomes of employment, capital expenditures, sales, and payroll per employee in the manufacturing sector, those outcomes are also separately accounted for by new establishments, previously existing establishments, and *vanishing* establishments, business establishments that have either closed or moved. Net changes can mask the churning of business establishment births and deaths that are common in both growing and shrinking economies (Davis, et al., 1996; Dunne, et al., 1989). In addition, sorting growth outcomes into the gross flows is important to properly account for the impacts of different policy features. For example, incentives in target areas such as newly equipped industrial parks may be focused on attracting new establishments to areas close to pockets of severe economic and social distress. Central city industrial or business district incentives may instead be more focused on helping existing businesses to survive or grow. Thus, empirically

testing whether certain policy features best favor the attraction of new businesses rather than the retention of existing production activities helps inform the design of future interventions.

This paper, unlike much of the existing EZ literature, allows such testing to be performed by using plant level manufacturing data from U.S. Census Bureau's Longitudinal Research Database (LRD) to estimate the marginal impact of specific EZ policy features on the baseline growth rates of the four different economic outcomes separately sorted into outcomes accounted for by new, existing and vanishing establishments. The analysis is performed using data from 11 states with a two-step econometric model that yields impact estimates reasonably free of selection and omitted variable biases under plausible assumptions on the selection into treatment process. As noted by Peters and Fisher (2002), to comparatively analyze the impact of geographically targeted tax incentives, it is important to properly measure and control for the value to businesses of the incentive packages offered in the target areas. A measure of the monetary value of the EZ tax incentive packages is included in the econometric model based on figures provided by the "hypothetical firm" method embedded in Fisher and Peters' (1998) Tax and Incentive Model (TAIM) algorithm.

Results of the analysis show that state EZ programs do not have a positive mean impact on the net growth rates of employment, capital expenditures, sales or payroll per employee. However, the impact of the EZ programs reveal more complex dynamics when growth outcomes are separately sorted into the gross flows. Some of the state-specific policy features are associated with positive outcomes in new and existing establishments, findings that lead to well-defined policy recommendations to refine future geographically targeted business incentive programs. The null impact of the EZ programs on the net growth rates is accounted for by an EZ-induced increase in the rate of business failures in the target areas, offsetting the treatment-induced positive impact on the attraction of new businesses and on the growth of existing EZ businesses.

The reminder of the paper is organized as follows. Section 2 further discusses the enterprise zone concept and briefly surveys some recent literature. Section 3 describes the ZIP code level programmatic and outcome data. Sections 4 and 5 describe the econometric methods and the sensitivity analysis. Section 6 illustrates the results, and section 7 discusses the major findings of the paper in the context of how they can be used to inform future geographically targeted economic development initiatives.

2. State Enterprise Zones

In the United States, the term “enterprise zones” commonly applies to programs that target tax breaks and economic development incentives to a subset of economically distressed areas. There is debate as to whether incentives should indeed be targeted in such a way at places rather than at people (Gyorko, 1998; Ladd, 1994). Typical economic justifications for place-based interventions focus on various market failures that challenge the sustainability of particular locations. For example, inefficient labor markets may not adjust promptly to changes in economic opportunities. In urban areas, there may also be a “spatial mismatch” between the central city residence of underemployed minorities and inaccessible suburban job opportunities (e.g., Ihlanfeldt and Sjoquist, 1998; Kain, 1994). Broader externality arguments are also invoked to justify geographic targeting (Bartik, 2000; Gyorko, 1998; Sridhar, 2001). Firms’ location decisions based solely on perceived private costs and revenues often do not account for all of the associated collective costs. These costs may include urban sprawl and traffic congestion as well as environmental degradation and public health and safety costs associated with uneven development.

In practice, the programs implemented by the states as “enterprise zones” vary considerably in terms of incentives offered, eligibility criteria and zone size and number (Erickson and Friedman,

1991), and there is little consensus regarding use of the term (Brintnall and Green, 1988). We study a sub-set of 11 state enterprise programs that maintain focused geographic targeting in order to evaluate whether such targeting can be effective. In addition, there is growing evidence that targeting works best when the number of areas targeted remains limited (Anderson and Wassmer, 2000; Wilder and Rubin, 1996), so we exclude from our analysis state EZ programs that have “targeted” a large fraction of the state.

Despite the wide popularity of EZ programs, the available empirical evidence on their effectiveness at creating local economic growth is still contradictory (e.g., Boarnet, 2001; Boarnet and Bogart, 1996; Buss 2001; Greenbaum and Engberg, 2000; Papke, 1994, 1993; Wilder and Rubin, 1996). Certainly, some of the variability in outcomes is due to the wide differences in zone policy, especially since many of the studies only focus on single zones or states. As exhaustively reviewed by Wilder and Rubin (1996), early evaluation studies from the mid 1980s were mostly descriptive case studies (e.g., Jones 1985, 1987) that focused on measuring economic activity in single zone areas at pre- post-designation periods, with no attempt to empirically separate effects of zone designation from those from other exogenous economic trends. Subsequent EZ evaluation efforts have used outcome data retrieved from interviews with zone program and business officials (e.g., Erickson and Friedman, 1990a, 1990b; GAO, 1988; HUD, 1986). These studies produced differing results on program effectiveness and were criticized for biases due to the tendency of survey respondents to overestimate the outcomes attributable to EZ incentives (e.g., Bartik and Bingham, 1995; Boarnet, 2001; Wilder and Rubin, 1996). A number of more recent studies have used econometric models drawing upon empirical evidence from single states’ programs (Boarnet and Bogart, 1996; Dowall, 1996; Lambert and Coomes, 2001; O’Keefe 2004; Papke, 1993, 1994). These studies have the advantage that they are reasonably free from biases due to different initial conditions between the target areas and the rest of the national

and regional economy. Such evidence from a single state, however, does not allow for the generalization of findings to other places or times due the extensive heterogeneity of the state EZ programs.

To provide results with larger external validity, Bondonio (2002), Bondonio and Engberg (2000), Greenbaum and Engberg (2004), and Peters and Fisher (2002) have utilized longitudinal business data from the US Census Bureau to perform comparative econometric evaluation studies across states. Such studies point toward a negligible net impact of zone designation. Greenbaum and Engberg (2004) and Bondonio (2002), however, suggest that the zero net impact results may indeed derive from a positive treatment effect on attracting new firms in the target areas and a counterbalancing loss of business activity due to the treatment-induced acceleration of downsizing and closure of existing firms. This consistent with earlier findings that any positive impact from zones is mainly attributable to new firms and expansions (Erickson, 1992). Peters and Fisher (2002) also analyze the composition of economic growth and decline in EZ areas with a descriptive analysis of the flows of establishment births, deaths and relocations into and out of zones.

This paper builds on this research by investigating the impacts of EZ incentives on economic growth outcomes measured directly in the EZ areas and in their most immediate vicinities. Successful geographically targeted programs should boost economic growth in the assisted areas by either attracting new firms or helping existing firms to survive and grow in the zone areas. Empirical evidence of such increased economic development would be found in increased employment, payroll, sales or capital expenditures. Some states emphasize incentives that reward capital investments and others focus more on labor subsidies. Measured outcomes will depend not only on factor input elasticities (Papke, 1993), but also on factor substitution induced by the particular incentives. For example, “successful” zone capital incentives could potentially lead to greater zone investment and

output without any measured increase in employment if the incentives induce more capital-intensive production. Therefore, it is particularly important to examine the different outcomes in the context of the different policy features. The next section further describes these features and the data utilized to measure programmatic outcomes.

3. Data

This paper examines programs from ten states (California, Connecticut, Florida, Indiana, Kentucky, Maryland, New Jersey, New York, Pennsylvania, and Virginia) and the District of Columbia during the period 1982 to 1992. These particular EZ programs were selected based on the following criteria: location of zones in urban distressed areas and/or in abandoned urban industrial parks; limited geographic extension of the program; and competition among local communities to receive the EZ status. The 11 EZ programs target areas that, in general, qualify for zone consideration by virtue of economic distress as measured by per-capita income, unemployment, and poverty rates crossing certain thresholds. In some states, zone eligibility is also based on population loss, land availability, and building vacancy criteria. The bulk of the EZ incentive packages offered in the 11 sampled programs rely on relieving the tax burden on businesses by tying incentives to either the number of new jobs created or the amount of investment made in the zones. In some cases, tax incentives are also complemented by in-kind services and utility subsidies. A detailed description of each EZ program is available in HUD (1995).

The state-specific policy features of the 11 EZ programs are operationalized in terms of a small number of variables in order to facilitate empirical testing of the impacts of unique policy features. These policy variables, collected from official state and federal (HUD, 1997) documents and interviews

with zone officials, are described in Table 1. As shown at the bottom of the table, the 11 zone programs all began in the early to mid 1980s.

Table 1

The variable EZ_SIZE measures the fraction of each state's total land area that comprised ZIP codes containing enterprise zones. The top number reports this fraction for 1987 and the bottom number that for 1992. In many of the states, the zone programs grew in size over the five year period, which is consistent with findings from geographically targeted programs in general (Greenbaum and Bondonio 2004) and enterprise zone programs in particular (Peters and Fisher, 2002; Talanker, 2003). The variable is used to test whether program size affects zone effectiveness, as there is some evidence that programs with fewer zones have been more successful (Erickson, 1992; Erickson and Friedman, 1990b, 1991; Wilder and Rubin, 1996). Because zones vary greatly in size, we prefer to measure relative land area designated rather than the count of the number of zones. The next policy feature listed in Table 1, EC_PLAN, is a dichotomous variable that measures whether the submission of a strategic economic development plan is a prerequisite for zone selection. Development of such a plan requires the coordinated effort of local economic development parties, and it has been argued that this sort of strategic planning can lead to better subsequent zone outcomes (Wilder and Rubin 1996).

We employ two indicators to measure whether zone business incentives are tied to employment or capital investment requirements to test for possible incentive-induced substitution between capital and labor. JOB_REQ is a dichotomous variable that indicates whether tax incentives are tied to creating new employment, and CAP_REQ similarly measures whether zone incentives are tied to new capital expenditures. Six of the 11 programs have the jobs requirement, and five of the programs require evidence of capital expenditures.

Finally, the monetary value of the EZ incentives to businesses, EZ_VAL, is used to estimate and control for the marginal impact of the monetary value of each state's EZ incentives on economic growth. The measure is calculated using the hypothetical firm approach embedded in the TAIM software developed by Fisher and Peters (1998). Following Bondonio and Engberg (2000), the monetary value of EZ incentives to businesses is defined as the difference between the average internal rate of return (IRR) computed through the TAIM software of an investment in a new plant made by a set of "typical" firms in an EZ area and the IRR of the same investment made by the same firms in a non-EZ area within the same state. Calculating this within-state difference is useful because the monetary value local tax incentives is widely believed to affect firm's location decisions primarily at the margin among similar nearby locations (e.g., Bartik, 1991; Bostic, 1996; Wilder and Rubin, 1996). While Peters and Fisher (2002) have expanded and extended the model (TAIM^{EZ}), the TAIM algorithm available at the time of this analysis covered only five of the 11 states included in the data set, California, Kentucky, New York, Pennsylvania, and Virginia. The value of the incentives ranged from a 0.12 percent difference in the IRR in California to a more generous 0.74 percent difference in Virginia. Because this measure could only be calculated for a subset of five states, the outcome analysis was replicated for both the "large" sample of 11 states and the smaller sub sample of five states.

The location of zones is coded in terms of U.S. Postal ZIP codes. Zone ZIP codes were either directly provided by local program officials or were retrieved by comparing maps provided by those officials to the ZIP code boundaries displayed by geographic information systems (GIS). ZIP codes are a useful geographic size for measuring localized business activity because they can capture local demand factors such as population, income and wealth (Bingham and Zhang, 2001). ZIP codes are also the smallest geographic unit for which detailed business data is available. While zone boundaries

do not coincide with ZIP codes, they do not typically coincide with any other administrative boundaries, either. Therefore, a ZIP area is coded as an EZ-ZIP if it includes any portion of an actual zone. Although this decision rule is not ideal because zone boundaries that overlap small portions of neighboring and less distressed ZIP codes might lead to an underestimation of the zones level of distress (Greenbaum, 2004), methods to apportion the boundaries to ZIP codes also have drawbacks (Dowall, 1996). Further, since EZ programs are aimed at improving local economies, it makes sense to be able to test whether zone designation has an impact on both the EZ areas and their most immediate vicinities.¹ A further drawback to ZIP codes is that their boundaries change over time, and those changes are not well recorded. Utilizing stable plant location identification codes from our business data, historic ZIP code area changes were identified to create a set of constant ZIP codes that could be used to compare data over time. The last row in Table 1 shows that the number of ZIP codes that contain zones by the end of 1992 varies from two in the District of Columbia to 125 in California. Note that this is a measure of the number of ZIP codes rather than the number of actual zones. For example, the District of Columbia designated three zones that were contained in two ZIP codes.

Economic growth outcomes, in the form of employment, value of shipments, capital expenditures and payroll per employee are obtained from the quinquennial Census of Manufactures (CM) portion of the Census Bureau's Longitudinal Research Database (LRD).² The data include every U.S. manufacturing plant with five or more employees in years 1977, 1982, 1987, and 1992.³ One advantage of the CM data available from the LRD over the publicly available version is the ability to track establishment-specific characteristics over time because each establishment has a unique

¹ Below, we check whether our models are robust to alternative decision rules that only count a ZIP as an EZ ZIP if at least 25 percent or 50 percent of its land contains an EZ.

² The LRD was accessed from the Census Bureau's Carnegie Mellon Research Data Center in Pittsburgh, PA. Results and descriptive statistics from the LRD have been screened to insure that no confidential data are revealed.

³ At the time of the analysis, the 1997 CM_LRD data were not yet available to the public.

identification number. Thus, pre-post intervention economic growth measures can be properly sorted into gross flows accounted for by new (establishments operating for the first time at their present location), existing (establishments operating at the same location as in the previous CM panel) and vanishing-establishments (establishments no longer in operation in the same location in the subsequent CM panel). While we examine data only from the manufacturing sector of the economy, that is the sector most heavily targeted by EZ programs (Peters and Fisher, 2002).

Finally, EZ pre-designation demographic, income, poverty, unemployment and population density characteristics are retrieved from the 1980 Decennial Census STF3a files. The census tract level data were allocated to ZIP codes using the Mable Geocorr geographical correspondence engine that determines the degree of overlap between different spatial units (Blodgett and CIESIN, 1998). As reported in Table 2, ZIP codes that subsequently became designated as EZs had in 1980, on average, higher unemployment rates, poverty rates, percentage of minority residents, and population density, and lower per capita income than ZIPs that were never designated. These ZIP codes also had slower growth rates of employment and business establishments between the years 1977 and 1982, and they had fewer occupied housing units and lower average housing values in 1980.

Table 2

4. Method of Analysis

4.1 Baseline model

Impact estimates of the EZ incentives are retrieved through a “two-step conditioning on a propensity score” baseline model that has been recently used in other EZ evaluation studies (e.g., Bondonio and Engberg, 2000; Engberg and Greenbaum, 1999). The model combines both a propensity score

approach to evaluation (e.g., Rosenbaum and Rubin, 1983, 1984) and a panel-data fixed effects approach that eliminates the time unvarying unobserved ZIP-specific characteristics which may be potentially correlated with treatment assignment.

In the first step of the model, the probability that each ZIP code is designated as an EZ is estimated as a function of the pre-designation employment and establishment growth and the 1980 socio-economic characteristics. Ideally, designation probabilities would be estimated by a separate regression for each state to better take into account that each state has unique designation criteria. However, the aggregation of zone boundaries to the ZIP code level leaves several of the states with too few observations on the dependent variable to estimate separate regressions. Instead, the probability of zone designation is estimated on two clusters of states.

The states are clustered based on the criteria mentioned in the state EZ legislations for selecting zone areas. The first cluster of states share zone selection guidelines that primarily include income, unemployment or poverty indicators: California, Connecticut, the District of Columbia, Kentucky, New Jersey, and Pennsylvania. In addition to the unemployment, income or poverty indicators, the second cluster of states share designation criteria that include measures of land availability or building vacancy: Florida, Indiana, Maryland, New York, and Virginia. For this second cluster of states, the probit specification also adds two 1980 housing market variables. The probit regressions for the two clusters of states are illustrated in equations (1) and (2):

$$P(EZ_i=1) = \Phi(\beta_1 UNEMP_i + \beta_2 POV_i + \beta_3 INC_i + \beta_4 DENS_i + \beta_5 MIN_i + \beta_6 EMPGRW_i + \beta_7 ESTGRW_i + \phi_1), \quad (1)$$

$$P(EZ_i=1) = \Phi(\beta_1 UNEMP_i + \beta_2 POV_i + \beta_3 INC_i + \beta_4 DENS_i + \beta_5 MIN_i + \beta_6 EMPGRW_i +$$

$$\beta_7 \text{ESTGRW}_i + \beta_8 \text{OCCHOUS}_i + \beta_9 \text{VALHOUSE}_i + \phi_{II}), \quad (2)$$

where:

i = ZIP codes;

$\text{UNEMP}_i, \text{POV}_i, \text{INC}_i, \text{DENS}_i, \text{MIN}_i$ = set of 1980 Census variables capturing unemployment, poverty, per capita income, population density and percentage of African Americans and Hispanics;

$\text{EMPGRW}_i, \text{ESTGRW}_i$ = 1977-1982 CM measures of employment and establishment growth;

$\text{OCCHOUS}_i, \text{VALHOUSE}_i$ = 1980 Census variables expressing the percentage of occupied houses and the average value of owner occupied houses;

ϕ_I = set of states dummies for cluster I (CA, CT, DC, KY, NJ, PA for the large-sample analysis; CA, KY, PA for the small-sample analysis);

ϕ_{II} = set of states dummies for cluster II: (FL, IN, MD, NY, VA for the large-sample analysis; NY, VA for the small-sample analysis).

The predicted probabilities from equations (1) and (2) are referred to as propensity scores (Heckman et al., 1997, 1998; Rosembaum and Rubin, 1983, 1984) and can be interpreted as a single ZIP-specific parameters that summarize all of the pre-intervention economic growth and socio-economic measures in a single index.

In the second step of the model, the predicted probabilities are inserted into the 5-year growth rate outcome regression of equation (3):

$$\ln(Y_{it}/Y_{it-5}) = \phi_j + \gamma_t + \alpha \text{EZ}_{it} * T_{it} + \sum_{pol} \phi_{pol} (\text{EZ}_{it} * \text{pol}_{it} * T_{it}) + \sum_c \delta_c \text{PR}_i^c + u_{it} \quad (3)$$

Where:

$$t = 1987, 1992;^4$$

$$i = \text{ZIP};$$

$$c = \text{cluster of states I or II};$$

$$j = \text{state};$$

$$pol = \text{EZ_SIZE, EC_PLAN, JOB_REQ, CAP_REQ, EZ_VAL}^5;$$

$$\text{Ln}(Y_{it}/Y_{it}) = \text{outcome growth rate in the 5-year period ending in year } t;$$

$$\begin{aligned} \text{EZ}_{it} &= 1 \text{ if ZIP } i \text{ contains an EZ which was active in the 5-year period ending in year } t; \\ &= 0 \text{ otherwise;} \end{aligned}$$

$$\begin{aligned} T_{it} &= [(t-t_i^d)/5] = \text{portion of the 5-year period ending in year } t \text{ in which ZIP } i \text{ contained an active} \\ &\text{EZ where } t_i^d = \text{year of EZ designation in ZIP } i; \end{aligned}$$

$$\begin{aligned} \sum_{pol} \phi_{pol}(\text{EZ}_{it} * T_{it} * pol_{it}) &= \text{set of interaction terms between EZ status, portion of the 5-year period} \\ &\text{in which ZIP } i \text{ contained an active EZ and one of the policy variables.} \end{aligned}$$

$$\sum_c \delta_c PR_i^c = (\delta_I PR_i^I + \delta_{II} PR_i^{II}) = \text{predicted probabilities from equations (1) and (2);}$$

$$\phi_j = \text{set of state dummies};$$

$$\gamma_t = \text{5-year period dummy [=1 if the } (t-(t-5)) \text{ period is 1982-87; =0 otherwise];}$$

$$u = \text{normally distributed error term.}$$

In equation (3), the program intervention is operationalized with a treatment status variable, weighted by the portion of the 5-year period in which the EZ was actually active, and a set of policy terms

⁴ As the CM data are available only at 5-year intervals for the years 1977-1992, the period 1977-1982 cannot be used in the analysis because it is not possible to sort out outcomes by type of establishments for 1977 (which would only be possible by comparing the 1977 data with the unavailable 1972 data). For this reason, the 5-year periods available for the analysis are only (1982-1987) and (1987-1992), and, therefore, t is either 1987 or 1992.

⁵ These are the policy variables listed in Table 1.

$[\phi_{EZ_SIZE}(EZ_{it} * T_{it} * EZ_SIZE_{it}), \dots, \phi_{EZ_VAL}(EZ_{it} * T_{it} * EZ_VAL_{it})]$ that interact the EZ status with each of the five state-specific policy variables.⁶ These interaction terms are included in the model specifically to test whether state-specific EZ policy features have an impact on the dependent variable. The propensity scores estimated from equations (1) and (2) are operationalized as two variables: PR^I and PR^{II} . The variable PR^I is constructed as the predicted probability from equation (1) for all the ZIPs located within the states included in cluster I and equals zero for the ZIPs located in any of the other states. Likewise, PR^{II} contains the predicted probability from equation (2) for all the ZIPs included in cluster II and zero for all other ZIPs.

The two propensity score variables control for all of the observed ZIP-specific pre-intervention growth trends and socio-economic characteristics. As shown in Table 2, EZ areas tend to have more disadvantaged socio-economic conditions and slower growth trends prior to the beginning of the programs than non-EZ areas. Such disadvantaged initial conditions could induce EZ areas to grow more slowly than non-EZ areas absent the program intervention. Without properly controlling for such differences, impact estimates of the program intervention could be wrong due to this selection bias. The model of equation (3) sorts out impacts due to observable differences in pre-intervention growth trends and socio-economic characteristics (coefficients δ_1, δ_2) from the actual impacts due to the program intervention and the specific EZ policy features (coefficients α, ϕ). Alternatively, each of the pre-designation characteristics could be directly inserted in equation (3) instead of being used in the two-step propensity score procedure. The two-step procedure has the advantage that it constitutes a convenient way to deal with non-linearities in the relationship between the dependent variable growth rates and the pre-designation characteristics. Failure to properly specify a model including all of the non-linearities in a one-step procedure would lead to a biased estimate of the treatment coefficient.

⁶ The exact composition of the set of policy interaction term varies across specifications as illustrated in Table 3.

Further, such a specification is difficult to model because economic theory does not offer solid guidance in this matter. Rosenbaum and Rubin (1983) demonstrate that conditioning on the propensity scores corresponds to conditioning on the correct functional form of the pre-designation variables in a direct regression of the outcome variable on the pre-designation variables.⁷

Estimating the propensity score values also allows ZIP areas with very low or high designation probabilities to be excluded from the estimation sample. In a matching-estimator setting, Heckman et al. (1997) and Dehejia and Wahba (1998) show that better impact estimates can be retrieved by limiting the estimation sample to observations with propensity scores for which there are both treatment and comparison observations. In this paper, the estimation sample is restricted by excluding the ZIP codes with propensity scores within the 1st percentile of the EZ-ZIPs' distribution or within the 99th percentile of the non-EZ ZIPs in order to reduce the influence of possible extreme outliers and alleviate the burden on the model to control for extreme pre-intervention characteristics.

The model of equation (3) also controls for unobserved time-unvarying ZIP-specific characteristics that may be correlated with treatment assignment. This can be seen by considering that equation (3) is obtainable by applying a panel data fixed-effect estimator (implemented through a long differencing transformation of the data)⁸ to the following outcome regression model:

$$Y_{it} = \phi_j + \gamma_t + \alpha * t * (EZ_{it} * T_{it}) + t * [\sum_{pol} \phi_{pol} (EZ_{it} * pol_{it} * T_{it})] + t * [\sum_c \delta_c PR^c_i] + \alpha_j + u_{jt} \quad (4)^9$$

⁷ Heckman et al. (1997) have argued that what claimed by Rosenbaum and Rubin (1983) does necessarily apply when the propensity score has to be estimated instead of being known with certainty. In such cases, according to Heckman et al., the conceptual and mechanical advantages of using a two-stage propensity score approach lies mostly in simplicity of estimation, mimicking the conventional econometric approach of using Mills ratios to correct for selection bias.

⁸ Long differencing is used in place of the more standard differencing from the mean or first differencing procedures due to the CM-LRD data being available only at 5-year intervals.

⁹ Coefficients of equation (4) are to be considered one fifth of those of equation (3) in order to allow exact correspondence

Compared to a standard panel data fixed effect estimation (in which time-unvarying pre-intervention characteristics would have no role), the two-step model of equations (1-3) yields unbiased impact estimates of the treatment under weaker assumptions. Standard fixed effect estimation is unbiased only assuming that all of the time-unvarying pre-intervention ZIP specific characteristics (that may be correlated with treatment assignment) have the same impact on the future levels of the dependent variable. Our model, instead, allows unbiased impact estimates to be retrieved even if the observed ZIP-specific pre-intervention characteristics were correlated with treatment assignment and affect the dependent variable not just by impacting its future levels in the same way but also by impacting its future growth trend. It should be noted, however, that our estimation procedure would yield biased impact estimates in the unlikely event that program officials designate EZ areas based solely on information unknown to the evaluator that would forecast which areas would otherwise grow the least or the most.

If such unobservable ZIP-specific growth trends (that could be formalized, for example, by adding a simple linear term such as β_{it} to equation 3) were correlated with treatment assignment (i.e., $\text{Cov}(\beta_{it}, EZ_{it}) \neq 0$), only random growth rates models (Boarnet and Bogart, 1996; Bondonio and Engberg, 2000; Heckman and Hotz, 1989; and Papke, 1993, 1994) would yield unbiased impact estimates of zone designation and EZ policy features. Random growth rates models are not feasible here due to the limited number of observations across time. However, the assumption of $\text{Cov}(\beta_{it}, EZ_{it}) \neq 0$ should not be too worrisome for the EZ programs analyzed in this paper: Even assuming that program officials would want to designate exclusively the best or worst future performers, their forecasting would likely have to be based on the same data and similar analytic tools available for this analysis.

between equations (4) and (3).

Finally, other methods, such as instrumental variables (IV) and Mills Ratios (Heckman, 1976; Heckman and Robb, 1985; Lee, 1978), are also not applicable to this analysis because they require a subset of variables that affect EZ designation but do not affect the baseline growth of an area.¹⁰ For EZ programs, unfortunately, such variables are quite impossible to find because zone areas are designated based on the very same pre-intervention economic and social conditions that represent some of the major factors affecting the post-intervention economic growth of zones.

4.2 Model specifications

The coefficient estimates for the model of equation (3) are estimated for the four different economic growth measures considered in the analysis across three different gross flows and overall net changes and account for heterogeneity by using robust cluster estimators that adjust the coefficient standard errors for the possible within-state correlation of observations (Rogers, 1993; Williams, 2000; Wooldridge, 2002). To compare the findings of this paper with those from other EZ studies, all sixteen specifications are first estimated without including any of the policy interaction terms other than the monetary value of the incentives, which is included only in the small-sample analysis. The analysis is estimated differently for the large sample of 11 states and for the smaller sample of five states that have information on the monetary value of the incentives. For the larger sample, all four policy interaction terms that are not formed with the monetary value of the incentives ($\phi_{EZ_SIZE}(EZ_{it}*T_{it}*EZ_SIZE_{it})$, $\phi_{EC_PLAN}(EZ_{it}*T_{it}*EC_PLAN_{it})$, $\phi_{JOB_REQ}(EZ_{it}*T_{it}*JOB_REQ_{it})$, $\phi_{CAP_REQ}(EZ_{it}*T_{it}*CAP_REQ_{it})$) are included in each specification. For the small-sample analysis, only two policy interaction terms are included at a time, one of which is always the monetary value of the EZ incentives. This is a choice

¹⁰ Mills Ratio methods need exclusion restrictions similar to those for IV models in order to obtain robust estimates (Robinson 1989).

made due to the limited variation in state-specific EZ policy features. A complete description of all specifications is illustrated in Table 3.

Table 3

5. Specification Tests and Sensitivity Analysis

Equation (3) is restrictive in two major ways. First it imposes that zone designation has the same impact on the outcome growth of interest in each 5-year period after designation without distinguishing between zones designated 5 years prior to time t and zones designated more than 5 years prior to time t . Second, it forces zone designation to affect the outcome growth of interest in each of the two 5-years periods linearly as a proportion of the 5-year period for which zones have been in existence. To test these restrictions, two less parsimonious and more generalized models are also analyzed. The first of these models is illustrated in equation (5):

$$\ln(Y_{it}/Y_{it-5}) = \sum_n \theta_n EZ_n_{it} + \sum_c \delta_c PR^c_i + \phi_j + \gamma_t + u_{it}, \quad (5)$$

Where

$$n = 1, 2, 3, 4;$$

$$EZ_n_{it} = 1 \text{ if } [(n-1)*2.5] \text{ years} < (t - t^d_i) \leq [n*2.5] \text{ years};$$

$$= 0 \text{ otherwise};$$

$$t = 1987, 1992;$$

$$t^d_i = \text{year of EZ designation.}$$

In equation (5), the EZ status indicator (EZ_{it}) is replaced by a set of four EZ status variables, (EZ_{1it} , \dots , EZ_{4it}), that reflect the age of the enterprise zone at time t measured at two and three-years intervals. EZ_{1it} equals 1 if ZIP i in year t is a zone in its first or second year of existence and zero otherwise. For example, when $t=1987$, $EZ_{1it}=1$ if ZIP i is a zone designated either in 1986 or 1985. When $t = 1992$, $EZ_{1it} = 1$ if ZIP i is a zone designated either in 1991 or 1990. EZ_{2it} , indicates whether ZIP i in year t is a zone in its third, fourth or fifth year of existence. Thus, when $t = 1987$, $EZ_{2it} = 1$ if ZIP i is a zone designated either in 1982, 1983 or 1984. When $t=1992$, $EZ_{2it} = 1$ if ZIP i is a zone designated either in 1987, 1988 or 1989. EZ_{3it} and EZ_{4it} , indicate whether a ZIP is a zone in its sixth-seventh and eighth-ninth-tenth year of existence, respectively. The model of equation (5) is more generalized than that of equation (3) because it allows zone impacts to vary freely over time at two- and three- year intervals and because it is able to differentiate the impact of a zone designated at the beginning of the five-year period $t-(t-5)$ from the impact of a zone designated in years prior to $t-5$. The model of equation (5) is implemented primarily to test the restrictions posed by the more parsimonious model of equation (3). Fully developing the model of equation (5) to include all of the policy features would require adding to the equation sixteen interaction terms, obtained by multiplying each of the four EZ status variables with each one of the four policy variables. The inclusion of these interaction terms would undermine the precision of any retrievable EZ policy feature impact estimate.¹¹

The specification of equation (5) can be used a way to test the two main restrictions posed by the equation (3). Although the restricted model of equation (3) is not perfectly nested into the model of equation (5), an F-test where the null hypothesis is that $\theta_3 = \theta_4$ (i.e., the coefficient on EZ_{3it} is equal to

¹¹ The fully developed model of equation (5) would be:

$$\ln(Y_{it}/Y_{it-5}) = \sum_{n=1,2,3,4} \theta_n EZ_{nit} + \sum_n \sum_{pol} \gamma_{n(pol)} (EZ_{nit} * pol_{it}) + \sum_c \delta_c PR^c_i + \phi_j + \gamma_t + u_{it}$$

$$EZ_{nit} = \begin{cases} 1 & \text{if } (n-1)*2.5 < (t-t^d_i) \leq n*2.5 \text{ years} \\ 0 & \text{otherwise} \end{cases}$$

Results from such specifications are available upon request to the authors.

that of EZ_4) can provide evidence on whether zones have the same impact on the dependent variable regardless of their previous age, provided that they are in place for the entire duration of the five-year period. Likewise, an F-test, where the null hypothesis is that $\theta_1 = (\theta_2 * 2/5)$ can provide evidence on whether zone designation affects the dependent variable linearly as a proportion of the five-year period for which zones have been in existence.

The second alternative model is illustrated in eq. (6):

$$\ln(Y_{it}/Y_{it-5}) = \sum_n \omega_n \{EZ_{it} * [t - (t_i^d + n)]/5\} + \sum_c \delta_c PR_i^c + \phi_j + \gamma_t + u_{it}; \quad (6)$$

where

$$n = 0, 3, 6$$

$$\begin{aligned} (t - t_i^d + n) &= 0 && \text{if } (t - t_i^d + n) < 0 \\ &= (t - t_i^d + n) && \text{if } 0 \leq (t - t_i^d + n) < 5; \\ &= 5 && \text{if } (t - t_i^d + n) \geq 5. \end{aligned}$$

In equation (6), the EZ status indicator EZ_{it} is replaced by a set of three EZ status variables ($EZ_{it} * [t - t_i^d]/5$, $EZ_{it} * [t - (t_i^d + 3)]/5$, and $EZ_{it} * [t - (t_i^d + 6)]/5$), that reflect the portion of the five-year period ending in year t in which a zone has been in existence measured from different times. $EZ_{it} * [t - t_i^d]/5$ expresses the portion of the five-year period ending in year t in which a zone i has been in existence, measured from the actual time t_i^d of zone designation. $EZ_{it} * [t - (t_i^d + 3)]/5$ expresses the portion of the five-year period ending in year t measured from three years after the time of designation t_i^d . Finally, $EZ_{it} * [t - (t_i^d + 6)]/5$ expresses the portion of the five-year period ending in year t measured from six years after the time of designation t_i^d . These three EZ status indicators allow the restrictions posed by the model of equation

(3) to be tested directly: If the coefficients (ω_3, ω_6) on the two EZ status variables $EZ_{it}*[t-(t_i^d+3)]/5$ and $EZ_{it}*[t-(t_i^d+6)]/5$ are both zero, equation (6) collapses into equation (3). Therefore, an F-test, where the null hypothesis is such that the coefficients ω_3 and ω_6 are both zero, tests directly if the restrictions posed equation (3) hold.¹²

Robustness of the results is finally also tested by replicating the model of equation (3) adopting two more stringent rules for sorting ZIP areas by EZ status: ZIP areas are coded as EZ ZIPs if at least 25, 50 or 75 percent of its land is covered by zones.

6 Results

Table 4 reports the results from the probit regression of equations (1) and (2). The coefficient estimates highlight the more important pre-designation characteristics affecting the probability of EZ designation for the 11 state large-sample analysis.¹³ The importance of the pre-designation characteristics varies across the two clusters into which the states are grouped. All states tend to designate areas with high proportions of minority residents and slow pre-designation employment growth. However, states in cluster I also target areas with low per-capita income and high population density, while states in cluster II tend to designate areas with high poverty rates and low housing occupancy rates and values.

Table 4

¹² Due to the limited variation in EZ policy features, the chosen specification allowing different growth rates after 3 and 6 years from designation is preferable to a less parsimonious specification which includes five different growth rates (from time of designation and after 2, 4, 6 and 8 years) and 20 interaction terms to operationalize the state-specific policy features:

$$\ln(Y_{it}/Y_{it-5}) = \sum_n \omega_n \{EZ_{it}*[t-(t_i^d+n)]/5\} + \sum_n \sum_{pol} \gamma_{n(pol)} \{EZ_{it}*[t-(t_i^d+n)]/5\} * pol_{it} \} + \sum_c \delta_c PR_i^c + \phi_j + \gamma_t + u_{it};$$

$n = 0, 2, 4, 6, 8$

Nevertheless, the analysis has also been replicated with such specification and results are available upon request to the authors.

To test the validity of the clustering technique adopted, the analysis has been replicated adopting the specification of equation (2), which includes two housing variables, for estimating the propensity score estimates of the cluster I states. Results from this specification, estimated on both the large and small samples of states, support the adopted clusters because the housing criteria do not affect zone designation for the cluster I states.

6.1 Impact of EZ status and monetary value of incentives

Results from the specifications without policy interaction terms are summarized in Table 5. The coefficient estimates can be interpreted as the marginal percentage point contribution to the baseline 5-year growth rate of zone designation and the generosity of the EZ packages. Generosity is measured by comparing the difference in internal rate of return (Δ IRR), as computed by the TAIM model, between a new investment in an EZ location and in a non-EZ location within the same state.

Mean impact estimates on the net economic growth outcomes (specifications I-IV) are reported on the top portion of Table 5. Results from the specifications are in line with much of the recent econometric empirical evidence on EZ programs showing a modest and statistically insignificant net impact of zone designation

Impact estimates of EZ status on employment, value of shipments and capital expenditure gross flows accounted for by new establishments show a positive marginal contribution of EZ designation to the 5-year baseline growth rates. These impact estimates range from 19.1 (specification VII, growth in capital expenditures) to 25.2 (specification V, employment growth) percentage points for the analysis on the large sample of states and from 10.3 to 29.3 percentage points for the small sample analysis.

¹³ Results for the five state small-sample analysis are very similar. They are therefore omitted for the sake of brevity and

Zone designation, by contrast, marginally depresses the baseline growth rate of the payroll per employee accounted for by the new establishments. The size of this negative impact is 32.4 and 35.6 percentage points for the large- and small-sample analysis, respectively. Impact estimates of zone designation on the baseline growth rates of employment, value of shipments and capital expenditures accounted for by existing establishments are also found to be positive (specifications IX-XII of Table 5), although the size of these estimates is much smaller than that on new establishment outcomes, ranging from 5.7 (specification X, growth in value of shipments) to 7.6 (specification XI, growth in capital expenditures) percentage points for the large-sample analysis and from 3.2 to 6.1 percentage points for the small sample analysis. No significant impact of EZ designation is estimated on the growth rate of payroll per employee accounted for by the existing-establishments. The final four specifications reported report the impact of zone designation on the 5-year rate-losses accounted for by vanishing establishments. In this part of the analysis, zone designation is found to significantly increase the baseline rate-loss of employment, value of shipments and capital expenditures in both the large and small sample analyses. EZ impact estimates for the payroll per employee specification, instead, do not reach statistical significance at conventional levels. For all specifications reported in Table 5, the estimated impacts of the monetary value of the EZ incentives in the small sample analyses have the expected signs but show large standard errors and fail to reach statistical significance.

Table 5

In general, these results prove to be quite robust, as they withstand the challenge of replicating the analysis with the alternative models of equations (5) and (6). Results from the model of equation

(5) are entirely consistent with the findings illustrated in Table 5. The two specification tests, $H_0: \theta_3 = \theta_4$ and $H_0: \theta_1 = \theta_2 * 2/5$, developed from equation (5) to check whether the restrictions posed by the model of equation (3) hold are in favor of accepting the restrictions. Results from the model of equation (6) show that estimating three different changes in the baseline growth rate measured at different times from zone designation do not provide any additional reliable information on the marginal impact of zone designation due to the loss in precision in retrieving the three slope estimates. The overall significance test on the three slope coefficients of equation (6) and their signs corroborate the findings retrieved from the model of equation (3). The specification test $H_0: \omega_3 = \omega_6 = 0$ from equation (6), which checks the functional restrictions posed by equation (3), also favors accepting such restrictions.¹⁴

6.2 Marginal Contribution of EZ Policy Features

Table 6 summarizes the results for the four specifications (V-VIII) of equation (3) estimated on the large sample of states that have new establishment gross flow dependent variables and that fully include the EZ policy features interaction terms.¹⁵ Similar to the results in Table 5, zone designation is found to positively affect employment, value of shipments and capital expenditure gross flows accounted for by new-establishments. Although zone designation appears to have a positive impact on new establishments, expansion of the program to more areas appears to counteract that effect since increases in the portion of the state designated as EZs significantly reduces growth rates across all four specifications. For each one-standard deviation increase in the land coverage of zones, there is a

¹⁴ Results for coefficient estimates and specification tests from all specifications of equations (5) and (6) are omitted for the sake of brevity and are available from the authors.

¹⁵ Results from the small-sample analysis are omitted for the sake of brevity. They are in all very similar to those from the large sample of states and are available upon request to the authors.

statistically significant marginal decrease in the 5-year baseline growth rates of -20.5, -17.5 and -14.6 percentage points for employment, value of shipments and capital expenditures, respectively.

Designating a larger portion (i.e. one standard deviation increase) of land covered by zones is also estimated to lower the growth rate of payroll per employee -43 percentage points on a 5-year base.

Table 6

Table 7 illustrates the impact estimates of the EZ status and the state-specific EZ policy features on existing-establishment gross flows for the large sample of states (specifications IX-XII).¹⁶ Estimates for the employment specification (IX) highlight that providing EZ incentives tied to the number of new jobs created is the only policy variable that marginally affects the employment baseline growth rate, with a positive marginal impact estimate of 22.2 percentage points on a 5-year growth rate. Tying incentives to hiring requirements is also found to be the only significant policy variable in the payroll per employee specification (XII), showing, however, a negative marginal impact of -31.2 percentage points. Mandating the provision of a strategic economic plan is the only policy feature significantly affecting the baseline growth rate of both value of shipments (specification X) and capital expenditures (specification XI), with positive marginal impact estimates of 25.8 and 17.5 percentage points, respectively.

Table 7

¹⁶ Again, results from the small sample analysis are omitted for the sake of brevity. Results from the small sample of states are very similar to those illustrated in Table 6 and are available from the authors.

Results for the vanishing-establishment specifications (XIII-XVI), not reported for the sake of brevity,¹⁷ highlight that, consistent with the results of Table 6, EZ status is found to marginally accelerate the rate-loss of employment, value of shipments and capital expenditures. None of the EZ policy interaction terms added to the model is found to significantly affect the baseline rate loss in any of the four estimated specifications.

All results reported in this section are robust in three ways. First, they are corroborated by the results from the specifications estimated on the small sample of states that allows the TAIM estimates of the monetary value of the EZ incentives to be included in the analysis. Second, they endure replicating the analysis by coding ZIP areas as EZ ZIPs only if they have at least either 25 percent, 50 percent or 75 percent of their land covered by actual zones. Third, the restrictions posed by the chosen functional form of equation 3 are accepted by the specification tests performed by replicating the analysis with the models of equations (4) and (5).

7. Conclusions and Policy Recommendations

This paper examines states' EZ experience of the mid 1980s to 1990s to test whether or not different impacts of EZ programs may be detected by looking at employment, sales (measured as value of shipments), capital expenditures and payroll per employee growth outcomes. These outcomes are evaluated separately for new-establishments, existing-establishments and vanishing-establishments. Testing whether EZ incentives differentially affect the various gross flows of local economic growth is important as some programs may focus on the attraction of new establishments while others may target the expansion or the retention of existing establishments. Knowing whether or not certain specific policy features are found to favor the attraction of new businesses instead of the retention of existing

¹⁷ Results for such specifications are available upon request from the authors.

production activities (or vice versa) may be an important piece of information to refine future geographically-targeted tax incentive initiatives.

Results show that when local employment, sales, capital expenditures and payroll per employee growth outcomes are sorted into different gross flows, EZ incentives do have some significant impacts. In particular, EZ policies are found to positively affect the gross flows of employment, sales, and capital expenditures accounted for by new-establishments. This positive impact, however, is counteracted by the overall territorial extension of the designated zones: Zones in states with a small geographic extension of zones tend to attract more employment and economic activity from new establishments than zones in states with a large EZ territorial extension. Changes in payroll per employee accounted for by new-establishments are negatively affected by the introduction of EZ incentives. Employment, sales and capital expenditures accounted for by existing establishments tend also to be positively affected by EZ policies, although by a smaller extent. For existing-establishment outcomes, however, tying zone incentives to job creation requirements is found to promote employment growth, while requiring the submission of an economic development strategic plan as part of the EZ application process promotes growth in sales and capital expenditures. EZ policies, finally, are found to significantly accelerate the rate-loss of employment, sales, and capital expenditures accounted for by vanishing establishments.

Similar to the empirical evidence of other recent econometric studies (e.g., Boarnet and Bogart, 1996; Bondonio and Engberg, 2000; Dowall, 1996; Greenbaum and Engberg, 2004; Peters and Fisher, 2002), mean impact estimates of the EZ incentives are found to not significantly affect any measure of net local economic growth. There are different possible explanations for these findings. New businesses could simply increase the rate at which previously existing businesses leave the area (Greenbaum & Engberg, 2004). In addition, new jobs and new economic activity are potentially much

more politically visible than retention of existing jobs and economic activity. New economic activity is used to emphasize the merit of EZ programs, while business closures are often unlikely to be linked to zone designation. As a result, zone incentives may tend to be marketed more toward attracting new establishments than toward helping struggling existing ones.

The greatest relevance of these findings, however, is perhaps related to the opportunity offered by the analysis to empirically test a number of predictions on recommended incentive features for geographically targeted economic development initiatives that stem from economic theory and/or interviews with business representatives and EZ program officials. Such predictions can be summarized as follows.

Incentives that reduce the price of capital goods may increase production and employment by lowering costs, but they may also have a substitution effect by inducing businesses to substitute capital for labor (Papke 1993). Programs that tie tax incentives awarded to zone businesses to the number of new jobs created, for example, can be viewed as more effective in promoting local employment growth than programs that tie incentives to capital investments (Wilder and Rubin 1996). Moreover, tax incentives are expected to appeal more to established businesses than to start-ups, since new businesses do not typically expect to make profits in the first years of operation (Sheldon and Elling 1989). Thus, programs that tie tax incentives to the creation of new jobs can benefit zone employment when the target of zone designation are primarily existing establishments. The results of the analysis presented in the paper support this prediction showing that tying incentives to new jobs created is the only EZ feature that marginally increases the baseline employment growth of existing EZ firms. Tying incentives to new jobs is, however, not found to have a significant impact on employment for either new firms or for outcomes other than employment. As EZ incentives are also not found to boost

payroll per employee, the results of the analysis suggest that the new jobs created tend not to be more skilled than existing jobs.

The provision of a strategic local economic development plan among the program application requirements may be important for effectively promoting zone employment and economic growth. For example, California program officials interviewed by Bostic (1996) revealed that the strategic planning portion of the application process was important to organize local development resources in a more productive way. In particular, mandating the submission of a strategic plan is viewed to be a key feature of EZ policies to effectively support existing firms. The development of an EZ strategic plan often brings together local business leaders with various administrative and community officials, thus facilitating communication regarding business needs and at the same time making business owners more aware of the opportunities offered by the EZ incentive packages.

Robust empirical support is offered to this prediction by the results of the analysis that show that the requirement of a strategic plan as part of the zone application process is beneficial to induce growth in sales and capital investment of existing establishments. The strategic plan requirement, however, does not significantly affect the attraction of new economic activity brought by new businesses. This may suggest that other efforts are necessary to inform potential investors about the availability of zone incentives.

According to program officials, the territorial extension of designated zones is the most relevant factor in boosting zone employment and economic activity brought by the attraction of new firms (Bostic 1996). In particular, EZ programs are viewed to be more successful in attracting new business if they adopt a competitive zone selection process that yields few designated EZs of limited size. EZ programs with limited zone territorial extension are advocated for three reasons. First, they allow more intense marketing efforts to each single zone (Wilder and Rubin 1996). Second, they enable program

officials to better evaluate the potential comparative advantage of the different eligible areas, yielding to the designation of areas that have developed the strongest local support for economic growth. Third, they facilitate close program monitoring and evaluation.

The results of the analysis support this prediction by showing that the baseline employment and economic growths accounted for by new establishments are positively affected by the reduction of the overall zone territorial extension. Reducing the territorial extension of the program is also found to increase the attraction of firms that offer higher levels of payroll per employee, suggesting that efforts to attract new businesses with qualified job opportunities are more successful if the size of the program is limited.

As state legislators continue to re-examine their enterprise zone programs, this paper offers some guidance based upon the experience of earlier programs. Within zone areas, greater attention should be paid to existing businesses, as the incentive programs studied in this paper benefited new establishments at the expense of established businesses. In particular, we find two policy features that have greater positive impacts on existing businesses than on new establishments. Job creation requirements appear to have some positive impacts in encouraging increased employment in existing businesses, and the requirement of a strategic local economic development plan appears to have induced increased sales and capital investment in existing establishments.

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Table 1. Policy Features of Enterprise Zone Programs

<i>Policy feature</i>	<i>Description</i>	<i>Measure</i>	<i>CA^a</i>	<i>CT</i>	<i>DC</i>	<i>FL</i>	<i>IN</i>	<i>KY</i>	<i>MD</i>	<i>NJ</i>	<i>NY</i>	<i>PA</i>	<i>VA</i>
Size of EZ program (EZ_SIZE)	Size of the program in terms of percentage of the total state land occupied by EZs in 1987 and 1992 ^b	% of state land occupied by EZs	1.26 3.45	0.8 2.75	0 14.55	0 9.33	1.67 3.62	2.51 3.22	4.79 10.03	6.59 6.59	0 2.31	0.21 6.46	5.37 7.91
Strategic plan required (EC_PLAN)	EZ designation follows a competitive application process requiring the submission of a strategic economic development plan by prospective EZ communities	1=yes 0=no	0	0	0	0	1	0	1	1	1	1	1
Tax incentives proportional to new jobs (JOB_REQ)	Program tax incentives based on the number of new jobs created by EZ firms	1=yes 0=no	1	1	0	0	1	1	1	1	0	0	0
Tax Incentives proportional to new capital investment (CAP_REQ)	Program tax incentives based on the size of the new capital expenditures promoted by EZ firms	1=yes 0=no	0	1	0	1	1	1	0	0	1	0	0
Monetary value to businesses of the EZ incentive package (EZ_VAL)	% point difference in the internal rate of return (IRR) between typical firms investing in a new plant in EZ and non-EZ locations within the same state ^c	Δ IRR: % point difference	0.12	-	-	-	-	0.22	-	-	0.18	0.23	0.74
	Program starting date	Year	1986	1982	1987	1987	1984	1983	1983	1985	1987	1983	1984
	EZ ZIPs ^d	Count	125	17	2	91	37	46	16	20	41	121	60

Notes:

^a California initially established two EZ programs that, respectively, established the Enterprise Zones and the Employment and Economic Incentive Areas. Because the two programs did not differ from each other in the policy dimensions considered in this paper, they are treated as a single program.

^b The top number measures the percentage in 1987 and the bottom number measures the percentage in 1992.

^c Δ IRR values vary across two-digit SIC industrial sectors. The values reported are state-specific average figures obtained by weighting each two-digit SIC specific estimate by the proportion of establishments operating in the state in that industrial sector prior to the start of the EZ program. Data are missing for CT, DC, FL, IN, MD and NJ because the available version of the TAIM software did not include those states at the time of analysis.

^d Number of ZIP code areas encompassing any portion of EZ areas in existence at December 31, 1992.

Table 2. Descriptive Statistics of Pre-Designation ZIP Code Characteristics:
11 State Sample

Measure	Variable	Mean (S.D.)	
		Zone ZIPs	Non-zone ZIPs
Unemployment rate, 1980	UNEMP	0.048 (0.018)	0.042 (0.027)
Poverty rate, 1980	POV	0.179 (0.103)	0.106 (0.068)
Per capita income (in \$1,000s), 1980	INC	5.968 (1.554)	7.253 (2.149)
Population density (1,000 people per km ²), 1980	DENS	1.856 (2.651)	1.015 2.957
Percentage of population black or Hispanic, 1980	MIN	0.307 (0.294)	0.111 (0.174)
Employment growth rate, 1977-1982	EMPGRW	-0.008 (0.732)	0.235 (0.997)
Establishment growth rate, 1977-1982	ESTGRW	0.561 (0.777)	0.799 (1.189)
Percentage of occupied housing units (occupied units/total units), 1980	OCCHOUS	0.877 (0.057)	0.893 (0.107)
Average value of owner occupied houses (in \$1,000s), 1980	VALHOUS	48.710 (24.367)	60.611 (32.064)

Notes:

Standard deviations are in parentheses.

Table 3 Model Specifications

Dependent variable: $\ln(Y_t/Y_{t-5})$	Specification	EZ Treatment Variables Included	
		11 State Sample	5 State Sample
A. Equations with no EZ policy interaction terms			
Net growth outcomes			
Employment	EMP	I	
Value of shipments	SHIP	II	
Capital expenditures	CAP	III	
Payroll per employee	PAYR	IV	
Outcomes due to new establishments			
Employment	EMP_NEW	V	
Value of Shipments	SHIP_NEW	VI	
Capital expenditures	CAP_NEW	VII	
Payroll per employee	PAYR_NEW	VIII	
Outcomes due to existing establishments		EZ*T	EZ*T
Employment	EMP_EXT	IX	and
Value of Shipments	SHIP_EXT	X	EZ*T*EZ_VAL
Capital expenditures	CAP_EXT	XI	
Payroll per employee	PAYR_EXT	XII	
Outcomes due to vanishing establishments			
Employment	EMP_VAN	XIII	
Value of Shipments	SHIP_VAN	XIV	
Capital expenditures	CAP_VAN	XV	
Payroll per employee	PAYR_VAN	XVI	
B. Equations with EZ policy interaction terms			
Net growth outcomes			
Employment	EMP	I	(EZ*T)
Value of shipments	SHIP	II	(EZ*T) and
Capital expenditures	CAP	III	(EZ*T*EZ_VAL)
Payroll per employee	PAYR	IV	
Outcomes due to new establishments			
Employment	EMP_NEW	V	Specifications replicated separately including one of the following groups of terms
Value of Shipments	SHIP_NEW	VI	
Capital expenditures	CAP_NEW	VII	
Payroll per employee	PAYR_NEW	VIII	
Outcomes due to existing establishments		(EZ*T*EZ_SIZE)	(EZ*T*EZ_SIZE)
Employment	EMP_EXT	IX	and
Value of Shipments	SHIP_EXT	X	(EZ*T*EC_PLAN) b) (EZ*T), (EZ*T*EZ_VAL),
Capital expenditures	CAP_EXT	XI	and (EZ*T*EC_PLAN)
Payroll per employee	PAYR_EXT	XII	(EZ*T*JOB_REQ)
Outcomes due to vanishing establishments		and	c) (EZ*T), (EZ*T*EZ_VAL),
Employment	EMP_VAN	XIII	(EZ*T*CAP_REQ) (EZ*T*JOB_REQ)
Value of Shipments	SHIP_VAN	XIV	
Capital expenditures	CAP_VAN	XV	d) (EZ*T), (EZ*T*EZ_VAL),
Payroll per employee	PAYR_VAN	XVI	(EZ*T*CAP_REQ)

Table 4. Probability of Zone Designation:
Probit Estimates from Equations (1) and (2) for 11 State Sample

Variables ^a		Cluster I CA, CT, DC, KY, NJ, PA	Cluster II FL, IN, MD, NY, VA
Unemployment rate, 1980	UNEMP	-1.888 (2.177)	-0.286 (1.202)
Poverty rate, 1980	POV	1.104 (0.721)	2.129*** (0.814)
Per capita income (in \$1,000s), 1980	INC	-0.084*** (0.031)	0.042 (0.044)
Population density (1,000 people per km ²), 1980	DENS	0.091*** (0.019)	-0.013 (0.014)
Proportion of population black or Hispanic, 1980	MIN	1.579*** (0.225)	1.387*** (0.229)
Employment growth rate, 1977-1982	EMPGRW	-0.127** (0.063)	-0.128** (0.058)
Establishment growth rate, 1977-1982	ESTGRW	0.017 (0.110)	0.049 (0.101)
Percentage of occupied housing units (occupied units/total units), 1980	OCCHOUS	-	-1.437*** (0.387)
Average value of owner occupied houses (in \$1,000s), 1980	VALHOUSE	-	-0.008** (0.003)
Number of observations		3068	2828
Pseudo R ²		0.2785	0.2736
Log likelihood		-830.42	-671.04

Notes:

* p-value<0.1 ** p-value<0.05 *** p-value<0.01

Standard errors are in parentheses.

^a For clarity of exposition, the coefficient estimates on the state fixed effects are not reported and are available upon request.

Table 5. Impact Estimates of Zone Designation on Five-Year Growth Rates:
Estimates from Equation (3) without the Policy Interaction Terms

Dependent variable (5-year growth rates) ^{a,b}		Model	11-state sample ^c	5-state sample ^d	
			Impact of EZ designation	Impact of EZ designation	Mon. Val of EZ incentive
<i>Net growth outcomes</i>					
Employment	EMP	(I)	0.019 (0.033)	0.031 (0.067)	0.047 (0.057)
Value of shipments	SHIP	(II)	0.012 (0.042)	0.014 (0.019)	0.053 (0.067)
Capital expenditures	CAP	(III)	0.035 (0.052)	0.024 (0.019)	0.036 (0.031)
Payroll per employee	PAYR	(IV)	-0.034 (0.037)	-0.074 (0.077)	0.222 (0.219)
<i>Outcomes due to new establishments</i>					
Employment	EMP_NEW	(V)	0.252*** (0.080)	0.293* (0.167)	0.260 (0.491)
Value of shipments	SHIP_NEW	(VI)	0.199** (0.101)	0.191* (0.102)	0.101 (0.216)
Capital expenditures	CAP_NEW	(VII)	0.191* (0.099)	0.103** (0.036)	0.233 (0.205)
Payroll per employee	PAYR_NEW	(VII)	-0.324*** (0.067)	-0.356*** (0.138)	-0.006 (0.408)
<i>Outcomes due to existing establishments</i>					
Employment	EMP_EXT	(IX)	0.067*** (0.028)	0.032*** (0.010)	0.146 (0.103)
Value of Shipments	SHIP_EXT	(X)	0.057*** (0.028)	0.061** (0.029)	0.102 (0.101)
Capital expenditures	CAP_EXT	(XI)	0.076** (0.046)	0.041*** (0.016)	0.156 (0.106)
Payroll per employee	PAYR_EXT	(XII)	-0.031 (0.040)	-0.099 (0.083)	0.290 (0.236)
<i>Outcomes due to vanishing establishments</i>					
Employment	EMP_VAN	(XIII)	0.192** (0.087)	0.205** (0.101)	-0.134 (0.106)
Value of shipments	SHIP_VAN	(XIV)	0.148* (0.108)	0.174* (0.103)	-0.110 (0.091)
Capital expenditures	CAP_VAN	(XV)	0.160* (0.103)	0.183* (0.104)	-0.098 (0.082)
Payroll per employee	PAYR_VAN	(XVI)	-0.102 (0.070)	-0.093 (0.145)	0.115 (0.248)

Notes:

* p-value<0.1 ** p-value<0.05 *** p-value < 0.01

^a For clarity of exposition, the coefficient estimates on the state dummies, the propensity scores, and the (1982-1987) five-year period dummy are not reported and are available upon request.

^b Prob>F = 0.000 (H₀: All coefficients=0) for all specifications.

^c N=11,766 for specifications (I-IV); N=8,284 (V-VIII); N=11,046 (IX-XII); N=7,622 (XIII-XVI).

^d N=7,852 for specifications (I-IV); N=5,368 (V-VIII); N=7,352 (IX-XII); N=5,086 (XIII-XVI).

Table 6. Marginal Impact of EZ Policy Features on New Establishment Outcomes:
Results from Specifications (V-VII), Equation (3)

Independent Variables ^a		Specification/Dependent Variable ^b			
		(V) EMP_NEW	(VI) SHIP_NEW	(VII) CAP_NEW	(VIII) PAYR_NEW
<i>ZONE DESIGNATION</i>					
Portion of 5 years with active EZ	EZ*T	0.692** (0.322)	0.756* (0.406)	0.687* (0.397)	0.299 (0.271)
<i>ZONE POLICIES</i>					
Strategic economic plan	EC_PLAN	0.134 (0.192)	0.025 (0.242)	-0.087 (0.236)	-0.478 (0.361)
Incentives tied to job creation	JOB_REQ	-0.308 (0.192)	-0.352 (0.241)	-0.426 (0.236)	-0.042 (0.161)
Incentives tied to capital expenditures	CAP_REQ	0.025 (0.177)	-0.164 (0.222)	0.053 (0.217)	-0.198 (0.148)
Portion of state land covered by zones [1=10% increase]	EZ_SIZE	-0.730** (0.315)	-0.641** (0.296)	-0.535 (0.387)	-0.493** (0.234)
Adjusted R ²		0.3306	0.3051	0.3037	0.2042

Notes:

* p-value<0.1 ** p-value<0.05 *** p-value<0.01

Standard errors are in parentheses.

N = 8,282

Prob>F = 0.000 (H₀: All coefficients = 0) for all specifications.

^a For clarity of exposition, the coefficient estimates on the state fixed effects, propensity score variables and time fixed effects are not reported. These are available from the authors.

^b F-tests of joint significance has been performed for all of the variables with the coefficient estimates having p-values > 0.10. For all four of the specifications, F-test results lead to failure to reject the null hypothesis of non-significance.

Table 7. Marginal Impact of EZ Policy Features on Existing Establishment Outcomes:
Results from Specifications (IX-XII), Equation (3)

Independent Variables ^a		Specification/Dependent Variable ^b			
		(IX) EMP_EXT	(X) SHIP_EXT	(XI) CAP_EXT	(XII) PAYR_EXT
<i>ZONE DESIGNATION</i>					
Portion of 5 years with active EZ	EZ*T	0.071 (0.156)	-0.083 (0.195)	0.235 (0.270)	-0.076 (0.165)
<i>ZONE POLICIES</i>					
Strategic economic plan	EC_PLAN	-0.060 (0.092)	0.258** (0.116)	0.175* (0.060)	0.165 (0.097)
Incentives tied to job creation	JOB_REQ	0.222** (0.092)	0.017 (0.115)	-0.110 (0.159)	-0.312*** (0.097)
Incentives tied to capital expenditures	CAP_REQ	-0.043 (0.085)	0.054 (0.107)	0.105 (0.148)	0.058 (0.090)
Portion of state land covered by zones [1=10% increase]	EZ_SIZE	0.057 (0.150)	0.145 (0.188)	-0.111 (0.261)	0.121 (0.159)
Adjusted R ²		0.3229	0.3293	0.3206	0.2104

Notes:

* p-value<0.1 ** p-value<0.05 *** p-value<0.01

Standard errors are in parentheses.

N = 11,046

Prob>F = 0.000 (H₀: All coefficients = 0) for all specifications.

^a For clarity of exposition, the coefficient estimates on the state fixed effects, propensity score variables and time fixed effects are not reported. These are available from the authors.

^b F-tests of joint significance has been performed for all of the variables with the coefficient estimates having p-values > 0.10. For all four of the specifications, F-test results lead to failure to reject the null hypothesis of non-significance.